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EQUIVALENCE RELATIONS

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When I was asked to reply to Horne and Lowe's (1996) criticisms of my position on equivalence relations, I replied that I did not hold to a *position*. In my recent book, *Equivalence Relations and Behavior: A Research Story* (Sidman, 1994; in the present article, subsequent references to chapters or to pages are citations of that book), I made many suggestions about how to view phenomena that are subsumed under *equivalence relations*. In each instance, I first detailed my reasons for making the suggestion. Then, I outlined experiments that might either support or fail to support the suggestion. If I have any position, it is that data rather than debate will show the way.

With one exception (Rumbaugh, 1995), most of the more important proposals in the book have received little theoretical and no empirical commentary, either from critics or from those who might be favorably inclined. I suggested, therefore, that I might simply reproduce selected paragraphs from my book. Somewhat to my surprise, this suggestion was received favorably. So here are some isolated paragraphs, repeated. Abstracting them from the general clutter will perhaps make them stand out more effectively.

Still, the surrounding material, although not included here, performs important functions, describing both the origins and possible consequences of each suggestion. Some proposals will not stand the test of data; for those, the originating problems will still remain. And so, I hope that anyone who really wishes to evaluate the following paragraphs will also attend to the context.

EQUIVALENCE AND THE REINFORCEMENT CONTINGENCY

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The study of equivalence relations has contributed some new data to behavior analysis and perhaps some new principles, but none

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of these requires the abandonment of data and principles that have already proven their worth. The enlargment of the analytic unit that is outlined in the *Emergent Verbal Classes* paper [Sidman, 1986] is just that—an *enlargement.* It encompasses equivalence relations and their contextual control but requires no fundamental change in the methods of analysis or in the underlying empirical and theoretical structure. In fact, I am convinced that the *Emergent Verbal Classes* paper provides a useful framework within which to organize the existing data and principles of behavior analysis.

Pages 324–325

It is true that laboratory research on equivalence relations was examining phenomena that behavior analysis had not previously considered. Our formulation of those phenomena had introduced a new set of terms to the behavior analytic vocabulary. To some, our introduction of new terms and concepts seemed to suggest that that we were discarding the old ones. In spite of the novelty of topic and terminology, however, I never felt that we were abandoning the system of behavioral anslysis that was founded on the experimental, theoretical, and philosophical contributions of B. F. Skinner. Rather, I viewed the work on equivalence relations as a natural extension of that tradition. A major aim of the *Emergent Verbal Classes* paper was to show that the equivalence relation, while perhaps a new behavioral concept as we had defined it, was an outgrowth of the same kind of contingency analysis that had given rise to basic relational concepts like stimulus, response, reinforcement, discrimination, conditioned and generalized reinforcement, and conditional discrimination.

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In [the Emergent Verbal Classes paper], I described the equivalence relation as emerging at the level of the four-term contingency. I now believe that this restriction of equivalence to the four-term unit placed too strong a constraint on the relation between equivalence and the units of behavioral analysis. Also, in the *Where Does Equivalence Come From?* paper [Sidman, 1990], I suggested that we have to consider seriously the possibility that equivalence is a basic stimulus function, not derivable from more fundamental processes. I now believe more strongly in this possibility. These developments in my conception of equivalence—the weakening of one belief and the strengthening of the other—are related, but definitive evidence is not yet in.

Pages 387–388

A terminological note. It must be recognized that to speak of the ''establishment'' of equivalence relations is a circumlocution . . . it saves words and eliminates awkward sentence constructions. *Equivalence relation* refers neither to a theoretical entity nor . . . to processes or entities that are beyond observation, but rather, summarizes a set of observed regularities. Strictly speaking, reinforcement contingencies do not *create* equivalence relations; rather, they create prerequisites, or the potential, for demonstrating the properties that define an equivalence relation. Additional factors, like the test conditions, contextual control, and a subject's behavioral history will help determine whether and how that potential is realized. . . .

An equivalence relation, therefore, has no existence as a *thing*; it is not actually *established, formed,* or *created.* It does not *exist,* either in theory or in reality. It is defined by the emergence of new—and predictable—analytic units of behavior from previously demonstrated units. . . .

The equivalence relation is not itself a unit of behavior from which more complex units are built. Nor is the equivalence relation a structure that is composed of more basic units. Although the diagrams that are typically used to depict experimental procedures may give a misleading impression of sequential or mediated learning processes, the definition of an equivalence relation does not require the component pairs to possess any temporal or structural property that might define a *mediating* event, a temporal or spatial *sequence,* an *association,* a *link,* an *associative link,* a *distance,* a *chain,* a *network,* a *conditioned stimulus,* a *conditioned response,* or any other kind of presumed basic structure or unit of behavior.

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In chapter 10, I pointed out that equivalence relations have been shown to include all possible ordered pairs of the stimuli in a

four-term analytic unit (conditional discrimination)—the conditional, discriminative, and reinforcing stimuli. Then, theoretical considerations along with some obtained and some anticipated empirical findings were advanced to support the inclusion of the unit's defined responses, too, among the components of the ordered pairs that make up an equivalence relation. These findings, actual and speculative, gave rise to the proposal that the contingency responsible for establishing the analytic unit is also responsible for the equivalence relation. . . .

Both of these proposals, (a) that defined responses be included as components of the equivalence relation's event pairs and (b) that the reinforcement contingency creates the equivalence relation, were said to be supported by findings that three-term contingencies . . . and perhaps even more restricted contingencies (response–reinforcer and stimulus–reinforcer) could also establish equivalence relations. If the four-term units that are needed for direct documentation of the . . . properties that define an equivalence relation can emerge from three-term (and perhaps two-term) contingencies that specify different reinforcers or defined responses, then, it was argued, reinforcers *and* defined responses would have to be included among the components of the relation. Only then could the emergence of equivalence from three- and perhaps two-term units be accounted for.

Pages 378–379

I am not calling for the inclusion of unobservable or invented responses in the equivalence relation. This is not just a repetition of mediation theory's practice of postulating the occurrence of responses in order to satisfy theoretical needs. Defined responses are neither invented nor inferred. They are . . . specified components of the reinforcement contingency. The contingency decrees that reinforcement be withheld unless the subject . . . emits the defined responses. . . .

The three-term unit and the definition of equivalence. The inclusion of defined responses as elements of event pairs that make up the equivalence relation turns out to be more than just an arbitrary assignment of labels. . . . One theoretically significant feature arises from the demonstration that discriminative stimuli can become related by equivalence even when they are involved only in three-term contingencies. This demonstration [Sidman, Wynne, Maguire, & Barnes, 1989] . . . calls into question our original behavioral definition of the equivalence relation. . . . In the face of demonstrations that three-term contingencies can generate equivalence . . . I was compelled to drop the notion that equivalence emerges only at the level of the four-term unit.

Because the direct evaluation of reflexivity, symmetry, and transitivity requires four-term units, abandonment of the four-term unit as the necessary origin of equivalence might cause one also to abandon the set-theory definition of the equivalence relation. This would be unfortunate. . . .

It turns out that the inclusion of responses in the equivalence relation not only permits but forces us to maintain our set-theory definition. This is because even though four-term units need not be involved in generating equivalence relations, it is still necessary to take the defining properties into account if we are to understand in principle how an inferred equivalence relation could have arisen from three-term units.

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In addition to making emergent conditional discriminations predictable [from smaller units], the inclusion of differential responses in the equivalence relation . . . permits us to escape the theoretical intricacies in which we involve ourselves when we hypothesize response mediation as the process responsible for emergent stimulus–stimulus relations. A major complexity of mediation theory is the requirement that the mediating responses must occur, although perhaps in a reduced form, whenever a subject demonstrates either baseline or emergent stimulus–stimulus relations. To maintain the necessary linear chain of stimulus–response–stimulus and so on, mediation theorists had to assume the occurrence of unobserved responses between each related pair of stimuli. This assumption leads to greater and greater awkwardness in the necessary explanatory constructions as derived relations come to involve more and more baseline nodes. The very inelegance of mediation theories of stimulus equivalence ought to occasion some skepticism as to their explanatory utility.

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If . . . we simply include defined responses as elements of event pairs that constitute an equivalence relation, we need postulate no linear mediating process in baseline or emergent relations, no unobserved responses, and no backward conditioning to account for symmetry. . . . By definition, the equivalence relation will include all of the stimulus–stimulus, stimulus–response, response–stimulus, and response–response pairs that are directly taught and all of the pairs that emerge in the tests. We need nothing more than our behavioral definition of equivalence to predict the emergent relations.

Pages 384–385

What does the inclusion of responses in the equivalence relation have to say about the distinction between stimulus and response? It says that *with respect to the equivalence relation,* such a distinction is unnecessary [emphasis added]. An equivalence relation is made up of pairs of events, with no restriction on the nature of the events that make up the pairs. The locus of those events, whether it be the living organism or the organism's living or nonliving environment, is irrelevant.

This does not mean there are no differences between stimuli and responses. Like all events, stimuli and responses can be members of many classes. . . . A classification that distinguishes stimuli from responses on the basis of their locus is meaningful, particularly when behavior analysts are trying to define their subject matter. . . .

In the context of describing the membership of an equivalence class, the distinction between stimulus and response, or even between controlling and controlled events, loses its significance. Ordered pairs of the events that comprise an equivalence class constitute the membership of an equivalence relation. How the individual events that make up these ordered pairs are classified in other contexts has no bearing on whether the pairs belong to an equivalence relation. An equivalence relation may contain stimulus–stimulus, response–response, stimulus–response, and response–stimulus pairs.

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In analyzing equivalence relations, then, we do not sometimes call an event a stimulus and at other times a response. Rather, we discard both of those terms. Equivalence relations have their own defining characteristics, none requiring the stimulus/response dichotomy.

The independence of equivalence relations from the arrows of time and causality removes any need to distinguish between stimuli and responses when specifying the membership of an equivalence class. . . . We need not conceptualize equivalence relations in terms that are relevant to conditioning.

Page 387

Still remaining is the question of where equivalence relations come from. An important part of the answer to this question almost leaps out at us now that we have included in the equivalence relation all the elements of the analytic unit. Question: Where does that unit come from? Answer: the reinforcement contingency creates the unit and with it, the equivalence relation. The establishment of equivalence relations is, then, one of the outcomes of reinforcement contingencies.

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Reinforcement contingencies select the particular elements that constitute a unit of analysis. As we have seen, the equivalence relation consists of ordered pairs of the unit's elements. The analytic units and the equivalence relations that reinforcement has established comprise an individual's repertoire of acts and discriminations. . . . The facts that analytic units and equivalence relations are established at all, however, are species characteristics. . . . the extent of the generality of [equivalence relations] across species is as yet unknown.

Pages 553–554

The origin of equivalence relations. Elementary mathematical set theory describes the abstract properties of equivalence relations, and I have argued that behavioral phenomena like those described in this book exemplify the mathematical abstraction. But although set theory informs us how to find out whether any particular event pair belongs to an equivalence relation, it is silent about the origin of equivalence relations themselves. Questions about testing for equivalence relations are to be distinguished from questions about prerequisites for the development of equiva-

lence. I have dealt with the latter problem by treating equivalence relations parsimoniously as a natural product of reinforcement contingencies. One consequence of this treatment is that all of the variables that modulate the effects of reinforcement contingencies can be expected to be found relevant also to equivalence relations, even though those variables may have no place in the mathematical description.

A second consequence is that no additional experience on the part of the individual need be invoked in order to account for the observation that the components of a reinforcement contingency are related by equivalence. My suggestion (chapter 10) was that equivalence relations are a built-in effect of reinforcement contingencies but that the breakdown of particular equivalence relations is a product of contextual control which, in turn, comes about through experience [Bush, Sidman, & de Rose, 1989]. . . . The problem then becomes not how to explain the origin of equivalence relations in general but how to explain those particular instances in which some or all of the events involved in a reinforcement contingency fail to become members of the same equivalence class. Experience is responsible for the removal or preclusion of events from an equivalence class.

Others, however, have argued that special kinds of experiences are necessary precursors of equivalence relations.

LANGUAGE AS A PREREQUISITE FOR EQUIVALENCE

Pages 362–364

Dugdale (1988) and Dugdale and Lowe (1990) have advanced the strongest arguments for the necessity of vocal or subvocal naming in the establishment of equivalence relations. Their studies demonstrated clearly that equivalence relations can be facilitated by naming or by some aspect of the experimental procedures or instructions that lead a subject to name the stimuli. This distinction between the effect of naming per se and the variables that lead to naming has not yet been addressed experimentally. At present, therefore, it is not clear that linguistic naming by itself has been the critical factor in these studies. . . .

In discussing the relation between naming and equivalence, Dugdale and Lowe (1990) proposed a distinction between *naming* and *labeling*. They recommended that we use the term *naming* only when the relation between the name and the thing named is symmetric. For example, true naming is demonstrated when a child not only says ''boy'' upon seeing a boy but, having said (or heard) ''boy,'' then points to a boy.

I find myself sympathetic to this suggestion . . . [but] I believe that Dugdale and Lowe's . . . definition of true naming is a component of a larger picture. In chapter 10, I will propose that differential responses in the analytic unit be included also in the equivalence relation. Such inclusion will require the relation between names and the stimuli that occasion them to be not only symmetric but reflexive and transitive as well. The equivalence relation will then include not only stimulus–stimulus pairs, but stimulus–response, response–stimulus, and perhaps even response–response pairs. Including differential responses in the equivalence relation will remove the necessity for the distinction that Dugdale and Lowe had to make between stimulus–response symmetry and stimulus–stimulus symmetry. This revised conception of the equivalence relation will also establish theoretical grounds for the facilitation of equivalence by differential responses (pp. 413–414).

Pages 306–307

In spite the interpretive difficulties that arise when naming tests are given after equivalence relations have been demonstrated, it would be imprudent to dismiss the naming data in Table 8-3 [from Sidman, Willson-Morris, & Kirk, 1986] too quickly. It is not obvious that all of those data can be attributed to subjects' misinterpretations of what they were being asked to do, or to other methodological features that might have caused the subjects to give different names than those they had applied to the stimuli during the earlier conditional-discrimination tasks. . . . [data review] . . . Taken together with earlier observations cited in the *Role of Naming* paper [Sidman et al., 1986] . . . and with later replications by Green (1990), these data cannot

easily be declared irrelevant to the question of whether common names are necessary to mediate equivalence relations.

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To say that verbal mediation is unnecessary for equivalence is not to say that verbal labels and rules are always irrelevant. To deny what Luria [as cited in Vocate, 1987, p. 135] has termed ''the abstracting and generalizing, analyzing and synthesizing power of language'' . . . would be contrary to everyday observation. But how does language help us to abstract, to generalize, to analyze, and to synthesize, and how does it come to do so? The mere acknowledgment of those powers does not explicate the role of verbalization. . . . It is possible, for example, that attaching labels to stimuli in a single-node equivalence class and expressing rules for relating those labels may help one subsequently to expand the class in accord with multinodal contingencies. Why verbalization should have such a facilitating effect is an interesting and important problem but its interest and importance are not restricted to equivalence phenomena. Surely, that facilitation involves something more than just the establishment of links in a causal stimulus–response chain (Skinner, 1957, pp. 107–129).

Pages 364–365

Generalized symmetry? Having postulated that true naming, a symmetrical stimulus–response relation, is necessary for stimulus equivalence, Dugdale and Lowe (1990) went on to ask where naming comes from. They pointed out that symmetrical stimulus–name relations arise naturally in the course of a child's language development, when the child is taught to be both a speaker and a listener—to say words and to comprehend those same words when others say them. Hayes, too (1991), has argued that such a history is necessary (although not sufficient) for equivalence relations. Dugdale and Lowe (1990) and Hayes (1991), therefore, attempt in this way to derive equivalence relations from an individual's linguistic experience. I believe, however, that they have overlooked a significant assumption that underlies their derivation. They assume that with enough name–event and event–name examples (which ordinarily occur extensively in a child's natural language community), a generalized relation of symmetry will emerge naturally.

As Hayes (1991) pointed out, the concern here is with arbitrary relations. . . . I can understand how a sufficient number of examples may give rise to generalized nonarbitrary relations like *larger, brighter, heavier, more,* and so on. But I do not understand how any number of examples can give rise to generalized arbitrary relations like *reflexivity, symmetry, transitivity,* and so on. Because the exemplars would possess no measurable feature in common, it is not at all evident that one might be able to generalize an arbitrary relation solely from exemplars. What aspect of several examples of symmetric event–name relations would permit a new example to be recognized or produced?

Symmetry is a complex verbal construction, involving preestablished classes like *names, things, self, others,* and so on. The mere exposure of a verbally unsophisticated organism like a child or a nonhuman to a number of exemplars that have, themselves, not yet been appropriately classified does not seem to me sufficient to explain the emergence of a generalized concept of event–name symmetry on the basis of any known behavioral principle.

The key here is ''known behavioral principle.'' . . . Mere exposure to exemplars may yet prove sufficient to yield a generalized concept of symmetry. . . . But, if classes defined by such relational properties can, like nonarbitrary classes, be generated merely by presenting exemplars to nonverbal or verbally unsophisticated individuals, this will itself define a new behavioral process, not derivable from anything more basic.

In attempting to derive equivalence relations from an individual's behavioral history, therefore, ''exemplar theory'' does not fulfill its intended purpose; it does not avoid the need to specify a behavioral process that is itself not derivable from anything more basic.

Pages 556–557

The accomplishments of mathematicians show us that linguistically proficient organisms can indeed abstract the properties of arbitrary relations and come up with a list of features that other similarly proficient organisms (behavior analysts?) can look for in any specific instance. . . . As I have asked before

(pp. 364–365), however, what makes it possible for linguistically unsophisticated organisms (like young children, people with severe mental retardation, or nonhumans) to abstract the shared features from a set of specific instances of reflexivity, symmetry, and transitivity when those very words—sophisticated abstractions that define the relation—remain outside of their repertoires? . . .

A linguistically naive organism's abstraction of commonalities from a set of exemplars that share no physical feature requires more of an explanation than just a history of experience with the exemplars. It is certainly possible to teach specific equivalence relations nonlinguistically, like *sameness,* for example, and to teach other kinds of arbitrary relations, too, like *opposition* and *difference*; all of these involve control by physical characteristics of stimuli. If, however, we were to find that linguistically impoverished organisms could derive the concept *equivalence relation* just from a reinforcement history with paired elements that shared no feature beyond the relation itself, that very finding would require an explanation that is not currently available among the principles of behavior analysis.

CONTEXTUAL CONTROL

Pages 512–513

Although the sources of equivalence have been a matter of theoretical dispute . . . the contextual control of equivalence relations has been generally agreed to have an experiential basis. I have gone so far as to suggest that experience may be required not to make equivalence possible but rather, to break down or prevent specific equivalence relations. . . . Instead of asking, ''Where does equivalence come from?'' I have found it useful to ask instead, ''What breaks down or precludes an equivalence relation?'' To answer this question in any particular instance, look for contingency-engendered contextual control.

Without experientially based contextual control, simple and conditional discriminations and equivalence classes . . . would be impossible; multiple class membership, giving rise to class union, would take events that we had to discriminate and bring them instead into one large equivalence class where they would all be treated alike. Everyday observation tells us, however, that events can belong to more than one class even while those classes remain independent of each other. In such instances, what breaks down or prevents class union?

Pages 523–524

The experiments I have just described [pp. 515–523] . . . show me clearly that contextual control does not create equivalence relations but rather, that context prevents lower level contingencies from generating potentially maladaptive equivalence relations, and breaks down equivalence relations that other contingencies have already generated.

Page 530

That is the background of the suggestion that was advanced in the *Contextual Control* paper [Bush et al., 1989] for a resolution of the problem of ''why the context itself does not become a member of all the emergent classes and, by virtue of its common membership, condense all of the classes into one'' (p. 507). Because equivalence relations are not directly specified in a reinforcement contingency, it is possible for a conflict to exist between the two outcomes of a contingency: (a) the creation of an analytic unit and (b) the formation of an equivalence relation. In a five-term unit, for example, . . . the contingency calls for differential control by discriminative stimuli in each three-term unit and by conditional stimuli in each four-term unit; on the other hand, the contingency creates equivalence classes containing a common contextual element that could wipe out differential control by bringing all discriminative and conditional stimuli together into a single class. Our suggestion was that creation of the unit takes priority. The explicit inclusion of differential stimulus control in the contingency counteracts the formation of equivalence classes; the latter are not only not explicitly included in the contingency but would actually prevent the conditions that are included from being met. And so, behavioral processes determine which aspect of the mathematically derived description is applicable; in this instance, whether control by context brings about class union or class intersection.

THE DESCRIPTIVE SYSTEM

Pages 536–537

My own theorizing has been directed not so much at an explanation of equivalence relations but rather, at the formulation of a descriptive system—a consistent, coherent, and parsimonious way of defining and talking about the observed phenomena. Mathematical set theory contains tools that allow me to meet all of these goals. . . . My colleagues and I therefore adapted set theory's definition of the equivalence relation, a definition that has a large number of regularities already built in. That is to say, the regularities themselves define the equivalence relation. Any relation that is to be called an equivalence relation *must* show those regularities. This necessity gives the descriptive system one of the flavors of an explanatory theory; it permits us to make predictions. The predictions, however, are already incorporated in the definition. . . . That those regularities have been so reliably confirmed continues to astonish me. . . .

In the course of writing the present story, I also found other components of mathematical set theory to be useful for the description of equivalence phenomena. In particular, the fundamental concepts of set union and set intersection permit us to include within the same descriptive system behavioral phenomena that had previously seemed to require the postulation of a separate process—*transfer* of function. . . . Different classes that possess members in common may merge into a single class—set union—or may remain independent—set intersection. Contextual components of the contingency determine whether set union or intersection takes place. None of this requires more than a description of the events that make up an observable reinforcement contingency.

Still, there is more to equivalence relations than mathematical set theory can describe. If equivalence relations are a product of reinforcement contingencies, all behavioral variables that are relevant to reinforcement contingencies must be relevant also to equivalence relations. Beyond this truism, some investigators have suggested that the mathematically derived description of equivalence relations is incomplete because new—previously unknown—behavioral variables or theoretical principles are involved. Any discovery of new variables is, of course, an exciting event. Nevertheless, such discoveries may introduce interpretive complexities and require difficult conceptual changes. It is wise, therefore, to follow a conservative course and search carefully for alternative explanations before trying to work a new variable into an existing formulation. Several instances in the literature on equivalence are worth noting.

Structural determinants: Directionality (pp. 537–538).

Structural determinants: Nodal distance (pp. 538–549).

Class size as a variable (pp. 549–550).

Page 550

The mathematics and the behavior. When applied to the analysis of behavior, the mathematical theory of sets seems to agree closely with behavioral reality. That this correspondence exists is in itself remarkable. How is it that purely mathematical conceptions fit observed behavioral phenomena so well?

The same question, of course, has been asked in physics and other natural sciences.

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Whitehead's conception that pure mathematics is concerned with general abstractions from matters of fact (see above) is also relevant to the sometimes expressed opinion that the mathematically derived behavioral definition of equivalence relations which I and my colleagues have offered is just ''Sidman equivalence.'' The implication is that the definition, if not capricious, is, at the least, arbitrary, with no stronger a priori justification than any other definition. Far from being arbitrary, however, the mathematical definition of the equivalence relation possesses tremendous generality: ''Equivalence relations are found not only in every corner of mathematics, but in almost all the sciences'' (Gellert, Küstner, Hellwich, & Kästner, 1977). To adopt the mathematical definition is to take the position that behavior is included among the many real-world specifics that the abstractions of mathematical set theory encompass. This position, although conceivably incorrect, is hardly arbitrary. Given the general empirical support for the mathematical formulation, the a priori denial of its relevance to behavior

is considerably more arbitrary than its acceptance.

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STANDARD PRINCIPLES, NONSTANDARD DATA, AND UNSOLVED ISSUES

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In their impressive article, Horne and Lowe (1996) reevaluate the theoretical import of stimulus equivalence. Their critical analysis seems largely correct. At one place, however, Horne and Lowe attribute the failure of equivalence explanations to the "artificial" character of match-to-sample procedures (Horne & Lowe, p. 238). In our view, the fundamental problem with the equivalence framework stems less from its reliance on artificial situations than from a lack of clear theoretical principles (cf. Harzem, 1995[1]). Were such principles available, the artificiality of the procedures involved would not constitute a significant obstacle; after all, most behavioral concepts, including the concept of reinforcement used by Horne and Lowe, have been derived from experimental

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